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Discussions

Interactive comment on "Peru upwelling plankton respiration: calculations of carbon flux, nutrient retention efficiency and heterotrophic energy production" *by* T. T. Packard et al.

Anonymous Referee #3

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This is a very interesting, well written and thought provoking manuscript that pursues the estimation of important and difficult to measure biogeochemical variables such as organic carbon flux and nutrient retention efficiency from estimates of plankton respiration derived from measurements of the ETS activity. Both the concept, calculations and results are novel, clearly presented and relevant for Biogeosciences. However I have some questions that would like to see discussed.

1) The model to estimate Fc from R (Eq.(1), Page 16184) is one-dimensional, which has several implications.

1.1. Lateral inputs of organic matter are assumed negligible for R compared to vertical

fluxes (Page 16183, line 17). This goes against abundant observations of relevant lateral POC and DOC transport in upwelling systems, which should be important here given the dynamics of the upwelling during the sampling, described in section 3. Even in less dynamic and less heterogeneous systems, the horizontal scale of the region supplying organic carbon to a water column should depend on the depth of the water column, which varies here from 63 to 4755 m. It is not clear which is the top area of the seawater cube where Fc is estimated (Page 16183, line 15), nor if it is different for shallow and deep stations. I think that it is important that the horizontal domain of the model is stated. The consequences of the assumption of irrelevance of lateral inputs should be discussed, and if possible quantified.

The assumption that DOC-based R is negligible (Page 16183, line 17) also goes against abundant empirical evidence, and hence requires an analysis of the error introduced in Fc estimations. This analysis should include potential biases in terms of both magnitude and variation because surely the relative contribution of DOC and POC to R will vary in space and time.

1.2. The one-dimensional model asumes that the relation between Fc and R is time-independent, or that the system is in steady state. However the temporal scales of Fc and R are not the same, and connection does not need to be instantaneous. The derived time dependence may be particularly important here, not only because of the dynamic upwelling, but because R is derived from ETS measurements where accumulated biomass (whose response to organic carbon inputs is slower than that of metabolic rates) may play a higher role than in situ activity. In a highly dynamic upwelling, high heterotrophic biomass may be related to previous organic carbon inputs, while a large Fc may occur together with low biomass. Given that the Fc should vary in time (and space) with productivity pulses, and that biomass evolution is not instantaneous, the potential limitations in estimating Fc from instantaneous vertical profiles of R should be discussed.

1.3. The model rests conceptually in the progressive consumption of organic carbon

associated to its vertical flux. However, this progressive consumption carries with it changes in composition and lability of organic matter. The composition of organic carbon should also vary along the horizontal scale. How these changes in composition and lability reflect on the relation between Fc and R along the vertical and horizontal axes?

2) A single equation is used to convert ETS measurements to RO2 though the water column, however I would expect this relationship to change with the composition and activity of the plankton community along the water column (e.g. with the relative abundance of auto/heterotrophic organisms, eu/prokaryotes, etc.), especially between the phytoplankton rich euphotic zone and the bathypelagic waters. This equation derives from the ETS:RO2 regression in Packard and Christensen (2004), which is based on 14 paired measurements from 4 stations in the Gulf of Maine (Packard and Williams 1981), where subsurface chlorophyll a maxima of 1.1 to 3.2 μgChla/L were found between 25 and 40 m depth, and surface values ranged 0.17 to 0.77 μgChla/L. Both the magnitude and distribution of Chla in the euphotic zone here (Figure 1b) are very different to those in the Gulf of Maine. Moreover, in Packard and Williams (1981) RO2 measurements were only made in the upper 10, 10, 9 and 25 m of each station, thus always excluding the DCM, and at depths where Chla concentration ranged from 0.09 to 0.94 ug/L. I feel that the use of this regression to convert ETS measurements to RO2 throughout the euphotic zone, mesopelagic and bathypelagic waters of the Peru upwelling needs to be justified. Are there other regressions available that may cover the spectrum of ecosystems, or at least the ranges of biomass included in this study?

3) The ranges of ETS activity in the regression of Packard and Christensen (2004) is 293 to 919 μg O2 L-1 d-1, however the range of ETS measurements here is 0.06 to 532 μg O2 L-1 d-1 (calculated from Table 3). 88 out of 102 measurements (Table 3) are out of the range in the regression, and all the data deeper than 100 m (29 measurements) are at least two to four orders of magnitude below the minimum value in the regression. The validity of the regression outside its range needs to be defended.

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Minor comments:

Page 16179, line 2, rather than net primary production, I think it should be net community production.

If ETS measurements and RO2 calculations were made throughout the water column, why in Fig.2.a only R in the EZ is based directly on the ETS measurements while the R in the aphotic zone below is based on the R models in Table 4 (Page 16183, line 8)? Are these modelled R estimates or the actual data what are used to estimate Fc from integrated R?

This manuscript makes a solid statement of the central position of plankton R in the connection of ecological and biogeochemical fluxes of energy, carbon and nutrients in the ocean, and from there explores new modes of estimating difficult to measure biogeochemical rates. This is important and timely, and a difficult undertaking because of the complexity of those connections across time and space. Although I ask the authors to explore and discuss these difficulties, and limitations derived from the necessary simplification in their model, their original approach is insightful and potentially important.